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A HISTORY OF PHYSICS.

A History of Physics. By Prof. Florian Cajori. Pp. viii + 322. (London: Macmillan and Co., Ltd., 1899.)

IT is a formidable undertaking to write a book on the history of physics beginning with the dawn of science and including helium and Röntgen rays, and the task does not become easier when the whole has to be condensed into the narrow limits of 300 pages. We possess a number of more extensive histories, such as Heller and Rosenberger, and a few most valuable biographies of eminent men. These are boiled down into the present volume, which is readable and generally accurate, but is too slight and superficial to serve any useful purpose. The study of the history of science is of the greatest importance to the scientific man, and in addition, it is, when properly treated, a most fascinating study; but when we come to estimate the value of any particular treatise, we must ask ourselves what it is precisely in the historical treatment that interests us. For when we wish to boil down our information we must know what has to be retained and what may be rejected, or else we should commit the error of imitating the manufacturer of meat extracts, who retains only the gelatine and water. That is, in my opinion, what Mr. Cajori has done. It may be his misfortune rather than his fault that the editor of NATURE has entrusted this review to me; but having always taken a considerable interest in historical questions, I must acknowledge the author's reading and learning while I question his judgment. That, of course, may be a matter of opinion. The value of historical studies seems to me to lie in two directions. It is of high interest to follow out the evolution of thought, the gradual development of ideas, from the first suspicion of a new truth to its final experimental proof or mathematical demonstration. The way especially in which the same idea constantly originates simultaneously in different minds, and the question how far the different view often adopted by different nations is purely a matter of education, or has some deeper cause, has never yet been fully developed.

But, quite apart from this, every reader of original papers knows how many points are always missed by subsequent writers, and how clear ideas are only obtained by a study of original sources. If, for instance, we only know Faraday, or Maxwell, or Hertz through modern text-books, however good these may be, we shall miss a large number of important suggestions. Led by these considerations, Prof. Ostwald has originated the reprinting of important treatises under the name of "Klassiker der exacten Wissenschaften," and in the announcement of this publication has regretted the neglect of historical studies.

Curiously enough, Prof. Cajori quotes this regret of Ostwald's, and expresses the hope "that the survey of the progress of physics here presented, may assist in remedying this defect so clearly pointed out by Prof. Ostwald." As a matter of fact, Ostwald's remedy for what he terms the "absence of the historical sense," lies in the reading of original memoirs, while Prof. Cajori's cure lies in the prescription of small homœopathic globules intended to contain the concentrated concoction of all that is essential; but whether this intention can be realised or not is exactly what is open to doubt. I must justify the opinion I have expressed by a few examples of Prof. Cajori's style and manner of treatment. I take in the first instance the reference to the second law of thermodynamics.

> "In February 1850, Rudolph Clausius (1822–1888) communicated to the Berlin Academy a paper on the same subject, which contains the Protean second law of thermodynamics: 'Heat cannot of itself pass from a colder to a hotter body.' . . . In March 1851, there appeared a paper by Wm. Thomson, which contained a perfectly rigorous proof of the second law. He obtained it before he had seen the researches of Clausius. The statement of this law, as given by Clausius, has been much criticised, particularly by Rankine, Theodor Wand, P. G. Tait and Tolver Preston. Repeated efforts to deduce it from general mechanical principles have remained fruitless. The science of thermodynamics was developed with great success by Thomson, Clausius, and Rankine. As early as 1852, Thomson discovered the law of dissipation of energy, discovered at a later period also by Clausius."

Apart from the fact that Clausius' paper of 1850 does not either contain the words quoted, nor any reference to a second law at all (though the substance of it is given in the paper), one would like to know to whom the information given in the passage can be of any use. If a student possesses no knowledge of thermodynamics, he will not be any the wiser by it; and if he does, he must have read books, such as Maxwell's "Theory of Heat," or Baynes' "Thermodynamics," and in any of these he will find much more ample and accurate historical references than those given by Mr. Cajori. In the same way throughout the book, the author generally does not go beyond what ought to be, and often is given in the ordinary text-books. As an example how short statements, though correct, often convey incorrect ideas, I give the account of the historical evolution of the ohm:

> "In 1861 the British Association and Royal Society of London appointed a committee, with Lord Kelvin at its head, to recommend a unit ('B.A.' unit). Weber's absolute unit of resistance, was a velocity. The British committee adopted this unit in principle. . . . The securing of a convenient unvariable resistance equal to 10^9 absolute units has been a difficult task. The B.A. unit was a little too small. The 'legal ohm' was provisionally adopted in 1883, by a committee appointed by the congress of 1881. It was the resistance at 0° C. of a column of mercury, 1 square millimetre in cross section and 106 centimetres long. Competent investigators, like Rayleigh and Mascart, contended that this column was a little too short, but some smaller values obtained by certain experimenters led to the adoption of the mean value of 106 centimetres. The 'legal ohm' satisfied no one, and failed to become legal in any country.
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> "Henry A. Rowland, after pointing out errors in some of the determinations previously made, found the length of the mercury column in question to be 106·32 centimetres."

As a matter of history, Rowland's paper of 1878, which is not mentioned, is of much greater importance than the later one, because it drew attention to the inaccuracy of the original determination of the British Association. There is no reason why Rowland's later investigation,

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published jointly with Kimball in 1884, should be singled out in the above passage, while Rayleigh is patronised as a "competent investigator."

In the history of the earlier times Prof. Cajori has followed good guides, such as Whewell and Mach; and though here also the space is entirely inadequate to give a sufficient account of the subject, we like this portion better than his treatment of contemporary science.

That there should be omissions was inevitable, but it might have been thought that the kinetic theory of gases was of sufficient importance to justify a short paragraph, while the only reference to that theory is to be found in the statement that a mathematical investigation of the radiometer action was given by Clerk Maxwell. Here again, as a matter of history, it was the discussion between Osborne Reynolds and Johnstone Stoney, not alluded to in the book, which brought about the correct explanation of the radiometer, and however important Maxwell's paper may be, it only appeared when the matter was cleared up. In a detailed historical account it is often necessary to allude to scientific squabbles and unpleasant discussions, but the author of this book might well have pleaded want of space, and omitted, for instance, such a passage as this: "William Thomson and Tait, placing a much lower estimate on Mayer's researches, brought the charge that Tyndall was belittling the work of Joule." I have marked several passages which are open to criticism, but it is not my intention to find fault with the details of a book which, as a whole, is perhaps as well done as could possibly be. It is the whole attempt to collect isolated facts in the belief that these constitute a history that seems to me to be mischievous. This does not, however, exclude the fact that most readers will probably find in the book some things they did not know before, and some useful references. ARTHUR SCHUSTER.

OUR SEA FISHERIES.

The Resources of the Sea, as shown in the Scientific Experiments to test the Effects of Trawling and of the Closure of certain Areas off the Scottish Shores. By W. C. McIntosh, M.D., LL.D., F.R.S., &c. Pp. xvi + 248, and Tables. (London: Clay and Sons, 1899.)

IT is well known that at the time of the late Lord Dalhousie's Royal Commission on Sea-Fisheries (1883–85) Prof. McIntosh conducted, for the Commission, a series of most important trawling investigations off the coast of Scotland which formed the starting-point of a good deal of the experimental and observational work of the Fishery Board for Scotland—work which has been noticed from time to time in the columns of NATURE during the last ten or twelve years. The book before us is practically devoted to the summing up of that work and the discussion of its results, and there is no one probably who has a better right to do that than Prof. McIntosh, who, by his trawling investigations in 1884, suggested the experiments of the Board, and who himself may be said to have superintended and controlled the work while acting as scientific member of the Board from 1892 to 1895—when he was succeeded by Sir John Murray. All this gives additional importance to the fact that Prof. McIntosh now declares against the policy of the Fishery Board, criticises their methods and their conclusions as published in recent annual reports, and is apparently in favour of removing all restrictions upon fishing, and of throwing the territorial waters open to trawlers and liners alike.

It is clear, then, that the book deals with debatable matters, and probably few fisheries experts will agree with the author in all his points. The work is happily named the "Resources of the Sea," as the central idea running all through it is that marine animals and plants have such extraordinary powers of reproduction as to be practically unaffected by the influence of man; while the secondary title shows that it is the trawling experiments and the results of closure of sea-areas off the coast of Scotland that are specially discussed and criticised.

There is an "Introductory" chapter, giving a general review of marine life, with most of the statements in which every biologist will agree. It may be remarked, however, that the vast possibilities of increase which may be true of diatoms and many groups of lower animals, and even of some fish, are not necessarily true of all kinds of food fishes in the inshore waters. The plaice and the sole are probably in this respect in very different case from the herring, the cod, and the oft-quoted haddock.

Prof. McIntosh next gives us a chapter dealing with the effects of trawling, and of the hooks of liners, upon the food, the eggs and the young of our fishes; the present state of the fishery steamers and their apparatus, and upon the method in which the Fishery Board for Scotland have carried out the recommendations of the Trawling Commission. In all of this there is naturally a good deal of evidence to show that the trawlers do not do the harm to the sea-bottom that has been from time to time ascribed to them; and we readily agree with the conclusion on p. 50:

> "A calm survey of the situation, therefore, does not lend support to the notion that the trawl, as ordinarily employed in sea-fishing, is the only destroyer of the invertebrate animals of the bottom; and, further, experience does not demonstrate that the sea-bottom in any known region has been, by the use of such line or trawl, so seriously impoverished as to be unable to support fish-life."

Some of us are unable, however, from what we know on other coasts, to endorse the further opinion that trawling does no great damage to the young food-fishes on the bottom. Our experience in Lancashire is that grave destruction of immature flat-fish is caused by trawling in the "nurseries" along the shallow sandy shores; and that in protected areas, such as the closed ground off Blackpool, a rapid increase of the more sedentary flat-fish takes place. We fail to understand the statement[1] on p. 234, that closure is powerless to prevent such destruction.

Prof. McIntosh then takes up, one by one, the sea-areas which have been closed against trawling by the Fishery Board, and in which experimental hauls have been made from time to time by the Board's small steamer, the *Garland.* Unfortunately that vessel is too small for the work; we labour under a similar dis-

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[1] "The capture of great numbers of small fishes by either trawlers or liners is a misfortune for the country, but the closure is powerless to prevent it." This requires further explanation.

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